that divides the bed of the heart-shaped Sea of Serenity can be easily traced. Anyone who has undertaken the preparation of a stereograph with the pencil or pen, knows how very difficult it is to avoid the production of roughness in the combined image at places where smoothness is desired. No two impressions from the same type can be taken that will not present some inequalities when stereoscopically examined, and no two groups of type representing the same sentence can be so accurately adjusted as not to betray imperfection when subjected to this searching test."

For this statement regarding the moon, I was subsequently criticized by an English writer, well known in astronomical circles, who considered it to be extravagant. The test furnished by the photographs of Jupiter is probably even more delicate than that afforded by photographs of the moon's minor inequalities of surface. The observation of "W. J. H." is certainly very interesting. By experiments made in 1882 I found that a plane binocular image became noticeably convex or concave when the pair of diagrams under examination were so disposed as to produce an angular retinal displacement of only 47″ (*Philosophical Magazine*, October 1882). By comparing the photographs of celestial objects whose distance is known, it may be possible yet to show that the minimum displacement measured in 1882 is really not quite a minimum. W. LE CONTE STEVENS.

22 Universitätsstrasse, Strassburg, Germany, February 4.

Notable Palæolithic Implement.

DURING the last five or six years I have lived at Dunstable, and many persons in the neighbourhood now know that I notice old things a little. The consequence is that various objects are now and again presented to me for purchase. These things are mostly no good—common fossils, pieces of "petrified water," shells, coins of the Georges, &c., but at times something worth notice comes to hand.

Late last autumn a number of stones of no value were brought to me; amongst them was a good, flattish, sub-triangular, Palæolithic flint implement which had been picked up in 1830 by a farmer named William Gutteridge on Dallow Farm, near Luton—the late Mr. Gutteridge's own land. The implement had been preserved by the farmer as a curious natural stone, and he had affixed a label to it with locality and date. The person of whom I secured the stone knew nothing of stone implements. I soon ascertained the name and date to be correct from a relative of the late William Gutteridge. In 1830 the Gutteridges had held Dallow Farm for over 150 years.

Dallow Farm is in the valley of the Lea, and three-quarters of a mile west of the river at Luton. The ground is, I think, about 50 feet above the Lea, and from 400 to 450 feet above the Ordnance datum, but the heights on the large-scale Ordnance map are here insufficient. I have never found a Palæolithic implement at Luton, but I have picked up a few drift flakes there, and found a good number of Palæolithic implements a few miles off.

The Dallow Farm Palæolithic tool was found by Mr. Gutteridge seventeen years before M. Boucher de Perthes published his discoveries in France (1847), and eleven or twelve years before he began to notice such objects.

The famous Gray's Inn implement was found in 1690; Mr. Frere's discoveries were made at Hoxne in 1800; the Dallow Farm implement comes next in 1830; and the Godalming implement (Evans, "Stone Implements," p. 529) about 1842.

Dunstable. WORTHINGTON G. SMITH.

Stereom.

AMONG wants long felt, at least by animal morphologists, is some word that shall express for Invertebrata the idea that the word *bone* expresses for Vertebrata. Words such as *skeleton*, *shell*, *test*, and *carapace* express the whole structure, not the substance of which it is made. Words such as *nacre* and *stereoplasm* express some particular form of hard substance strictly defined from a physical or morphological stand-point. *Sclerenchyma* is the only word that has yet been used in anything like the required sense; but that is confined to corals, and, from its affinity with *cœnenchyma* and the like, it is well that it should be so. Driven back on cumbrous periphrases, I therefore venture to suggest the adoption of the word *Stereom* (στερέωμα, that which has been made solid). This word was used by Aristotle ("De Anim. Part.," ii. 9) for the hard as opposed to the soft tissues of the body, and may, for the purposes of modern science, be thus defined: any hard calcareous tissue forming skeletal structures in Metazoa Invertebrata, and in Protozoa. F. A. BATHER.

February 9.

Destruction of Fish by Frost.

IN regard to Prof. Bonney's letter of January 26 (p. 295), I would ask whether the fish were not killed by want of air due to the stagnancy of the water in the canal?

The moat here abounds in fish, and several holes were kept open for their sakes during the frost. The first partial thaw set our land-drains running. Where one of these began to pour a little water into the moat, though no fish had been visible since summer, now the largest pike and carp were seen crowding to the aperture, seeming to be gasping for air, and seeking the fresh flow. When the frost departed, scarce half-a-dozen fish—all small—were found dead. It would seem, therefore, that a very slight flow of fresh water would suffice to save fish from death. But this can seldom be wanting in any natural body of water, for few are even the tarns into which no brook runs. So such a cause of destruction can seldom have acted on a scale visible to a geological eye. E. HILL.

The Rectory, Cockfield, Suffolk.

A DEDUCTION FROM THE GASEOUS THEORY OF SOLUTION.[1]

BEFORE passing on, let me briefly recapitulate the chief points in Van't Hoff's gaseous theory of solution and the experimental laws on which it it is based.

(1) In every simple solution the dissolved substance may be regarded as distributed throughout the whole bulk of the solution. Its total volume is therefore that of the solution, the solvent playing the part of so much space; and its specific volume is the volume of that quantity of the solution which contains 1 gramme of the substance. To avoid confusion, it is best to speak of this as the *specific solution volume* (v) of the substance. It is obviously in inverse ratio to the concentration.

(2) In every simple solution the dissolved substance exerts a definite *osmotic pressure* (p). This is normally independent of the nature of the solvent. It varies inversely as the specific solution volume (or directly as the concentration), and directly as the absolute temperature (T). We may then write for solutions, as we do for gases, the equation $p \,.\, v = r \,.\, \mathrm{T}$, where p and v have their specialized meanings, and r is a constant for each soluble substance.

(3) The *molecular solution volume* of all dissolved substances is the same if they are compared at the same temperature and osmotic pressure. If m be the molecular weight, $m \,.\, v = \mathrm{V}$ is the molecular solution volume; and we can now write, as we do for gases, $p \,.\, \mathrm{V} = \mathrm{R} \,.\, \mathrm{T}$, where R is the same constant for all substances.

(4) This constant R has the same value when the formula is applied to the dissolved state as when it is applied to the gaseous state itself.

(5) The gaseous laws, as I have stated them, are not absolutely true for dissolved matter in all circumstances. Dissociation often occurs, as it may occur in the process of vaporization, thus causing apparent exceptions. But apart from this there are and must be variations from the laws in the case of solutions of great concentration, just as there are in the case of gases and vapours of great concentration—for instance, in the neighbourhood of the critical point.

I wish now to ask your attention more particularly to the actual process of dissolving. and then to lay before you a hypothesis which, as it seems to me, is a logical consequence of the general theory.

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[1] Part of an address delivered by Prof. Orme Masson as President of Section B of the Australasian Association for the Advancement of Science, January 1891.

Imagine, then, a soluble solid in contact with water at a fixed temperature. The substance exercises a certain pressure, in right of which it proceeds to dissolve. This pressure is analogous to the vapour pressure of a volatile body in space, the space being represented by the solvent; and the process of solution is analogous to that of vaporization. As the concentration increases, the osmotic pressure of the dissolved portion increases, and tends to become equal to that of the undissolved portion; just as, during vaporization in a closed space, the pressure of the accumulating vapour tends to become equal to the vapour pressure of the liquid. But if there be enough water present, the whole of the solid will go into solution, just as the whole of a volatile body will volatilize if the available space be sufficient. Such a solution may be exactly saturated or unsaturated. With excess of the solvent it will be unsaturated, and the dissolved matter will then be in a state comparable to that of an unsaturated vapour, for its osmotic pressure will be less than the possible maximum corresponding to the temperature. On the other hand, if there be not excess of water present during the process of solution, a condition of equilibrium will be arrived at when the osmotic pressure of the dissolved portion becomes equal to the pressure of the undissolved portion, just as equilibrium will be established between the volatile substance and its vapour if the space be insufficient for complete volatilization. In such a case we get a saturated solution in presence of undissolved solid, just as we may have a saturated vapour in presence of its own liquid or solid.

So far we have supposed the temperature to be stationary, but it may be raised. Now a rise of temperature will disturb equilibrium in either case alike, for osmotic pressure and vapour pressure are both increased by this means, and a re-establishment of equilibrium necessitates increased solution or vaporization as the case may be.

Now what will this constantly increasing solubility with rise of temperature eventually lead to? Will it lead to a maximum of solubility at some definite temperature beyond which increase becomes impossible? Or will it go on in the way it has begun, so that there will always be a definite, though it may be a very great, solubility for every definite temperature? Or will it lead to infinite solubility before infinite temperature is obtained? One or other of these things must happen, provided, of course, that chemical change does not intervene.

Well, let us be guided by the analogy that has hitherto held good. Let us see what this leads us to, and afterwards examine the available experimental evidence. We know that a volatile liquid will at last reach a temperature at which it becomes infinitely volatile—a temperature above which the liquid cannot possibly exist in the presence of its own vapour, no matter how great the pressure may be. At this temperature, equilibrium of pressure between the liquid and its vapour becomes impossible, and above this point the substance can exist only as a gas. This is the critical temperature. And so it seems to me that, if we carry our analogy to its logical conclusion, we may expect for every substance and its solvent a definite temperature above which equilibrium of osmotic pressure between undissolved and dissolved substance is impossible—a temperature above which the substance cannot exist in presence of its own solution, or, in other words, a temperature of infinite solubility. This may be spoken of as the *critical solution temperature.*

But a little consideration shows that in one particular we have been somewhat inexact in the pursuance of our analogy. For we have compared the solution of a *solid* body to the vaporization of a volatile *liquid.* We can, however, do better than this, for volatile solid bodies are not wanting. It is to these, then, that we must look in the first instance. Now a volatile solid (such as camphor or iodine) will not reach its critical point without having first melted at some lower temperature; and a similar change should be exhibited in the solution process. At some definite temperature, below that of infinite solubility, we may expect the solid to melt. This *solution melting-point* will not be identical with, but lower than, the true melting-point of the solid; and for the following reason. No case is known, and probably no case exists, of two liquids one of which dissolves in the other and yet cannot dissolve any of it in return. Therefore there will be formed by melting, not the pure liquid substance, but a solution of the solvent in the liquid substance. Hence the actual melting or freezing point must be lower than the true one, in right of the laws of which I have spoken when discussing Raoult's methods in the earlier part of this address.

From this solution melting-point upwards we shall then have to deal with two liquid layers, each containing both substance A and solvent B, but the one being mostly substance A and the other mostly solvent B. These may be spoken of as the *A layer* and the *B layer.* As temperature rises, the proportion of A will decrease in the A layer and increase in the B layer; and every gramme of A will occupy an increasing solution volume in the A layer (B being absorbed there) and a decreasing solution volume in the B layer. At each temperature the osmotic pressures of A in the two layers must be equal. The whole course of affairs, as thus conceived, now admits of the closest comparison with the changes which accompany gradual rise of temperature in the case of a volatile liquid and its saturated vapour. The liquid is like the substance A in the A layer; the vapour (which is the same matter in another state) is like the same substance A in the B layer. As temperature rises, the liquid diminishes in total quantity, the vapour increasing; but the specific volume of the liquid increases, while that of the vapour decreases. The residual liquid is, in fact, constantly encroaching on the space of its vapour, just as the residual substance A in the A layer is constantly absorbing the solvent B from the B layer. Finally, in either case, the specific volume of the substance will become identical in both layers, which means that the layers themselves will become homogeneous and indistinguishable. Our system will then have reached its critical temperature—the temperature of infinite volatility in the one case and of infinite solubility in the other.

So much for hypothesis. Are there any facts in support of it? Well, in the first place the hypothesis demands that (in the absence of chemical change) increase of solubility with rise of temperature shall be as general a law as increase of vapour pressure; and we find that this agrees with the known facts, more especially since Tilden and Shenstone (Phil. Trans., 1884) cleared up certain doubtful cases. Secondly, the hypothesis seems to demand some connection between the true melting-points of salts and the rates of their increase of solubility; and such a relation has in a general way been established by the same observers. Thirdly, we have the fact, in complete accordance with the hypothesis, that while no case is known of a solid body having, as such, infinite solubility in any simple solvent, several cases are known of liquids of infinite solubility, and also of *solids which, after they have melted in presence of their own solution, become at some higher temperature infinitely soluble.* This last statement refers to the cases described by Alexéeff (*Wiedemann's Annalen*, 1886), of which I must say a good deal more directly. It would seem to apply also to the case of silver nitrate, which Tilden and Shenstone described as dissolving in water to the extent of 18·25 parts to one at so low a temperature as 130° C. The true melting-point of the salt is 217°; and I have seen it stated (but have been unable to find the published account) that Shenstone has himself shown it to be fusible in water, and of infinite solubility at quite reachable temperatures.

With regard to substances that are liquid under ordinary conditions, we have the well-known fact that some pairs are infinitely soluble in one another, while others exhibit the phenomenon of only partial solubility. The hypothesis would draw no hard and fast distinction between these cases, except the practically important one that such a mixture as that of ether and alcohol, which belongs to the first class, is usually above its critical solution point, while such a one as ether and water, which belongs to the second class, is usually below it. It should be possible, according to the hypothesis, to cool mixtures of ether and alcohol sufficiently to cause separation into two layers, similar to those observed at the ordinary temperature in the case of ether and water; but I do not know that this has yet been put to the test of experiment.

Alexéeff's experiments appear to me to be of the very highest importance, and to merit the closest attention in any inquiry into the nature of solution. As already stated, they afford the strongest support to the hypothesis which I have been discussing; indeed, had it not been for this support I should hardly have ventured to discuss it at all. They refer to solutions in water, below and above 100°, of phenol, salicylic acid, benzoic acid, aniline phenylate, and aniline, and to solutions in molten sulphur of chlorobenzene, benzene, toluene, aniline, and mustard oil. All these afford instances of reciprocal partial solution throughout a considerable range of temperature, leading eventually at a definite temperature to infinite solubility. Several of them afford instances also of solid substances with solution melting-points below their true melting-points.

Alexéeff experimentally determined the temperatures at which different mixtures of the same two liquids are just converted into clear solutions; or, in other words, he ascertained the strengths of the saturated solutions corresponding to different temperatures For each pair of liquids he found that when a particular strength of mixture is reached, the temperature of saturation is lowered by further addition of either liquid. Thus a mixture of about 37 parts aniline to 63 parts water requires a temperature of 164°·5 to convert it into a homogeneous solution; but one of 21 of aniline to 79 of water assumes this condition at 156°, and one of 74 of aniline to 56 of water does so at 157°·5. He plotted his results in the form of curves, with temperature and percentage strength as the two co-ordinates. The curve for aniline and water is shown in Fig. 1; and this may be taken as a fair representative, the general form of all being similar. It is at once apparent that for every temperature up to a certain limit there are two possible saturated solutions, one of water in aniline and one of aniline in water. The limiting temperature at which there is but one possible saturated solution, and above which saturation becomes impossible, is called by Alexéeff the *Mischungs Temperatur*. It is what I have called the critical solution temperature. It is in the case of aniline and water about 167°, as nearly as one can judge from the curve without a greater number of experimental points than we have in this part; and the corresponding saturation strength is about 50 per cent. It is hardly necessary to say that this equality of the two ingredients is an accident which does not characterize all cases.

Now imagine a 50 per cent. mixture of aniline and water sealed up in a tube, shaken, and gradually heated. Let us assume that the tube is only large enough to contain the mixture and allow of expansion by heat, so that evaporation may be neglected as too small to materially complicate the result. The course of events will be exactly what I have already described with reference to the hypothetical A layer and B layer. There will be formed a saturated solution of water in aniline, which we may call the *aniline layer*, and a saturated solution of aniline in water—the *water layer*. Given the temperature, the percentage strength of each layer may be read off from the curve. As the temperature rises, the two layers will effect exchanges in such a way that the aniline layer will become poorer, and the water layer richer in aniline; and at about 167° the two layers will have attained equal strength and become merged into one. Were we to start with the aniline and water in any other proportions by weight, there would still be formed the two saturated solutions, but their relative amounts would be different, and one or other would be used up and disappear at a lower temperature than 167°. To attain the maximum temperature of complete solution you must start with the exact proportions which correspond to that temperature.

But it is possible to learn even more from Alexéeff's work than he himself has made evident. Let me call your attention to the curve shown in Fig. 2,[1] the data for which I have calculated in the following manner.

From Alexéeff's percentage figures was deduced the weight of water capable of dissolving, or being dissolved by, 1 gramme of aniline at each of his experimental temperatures, so as to form a saturated solution. Then from curves showing the expansion of pure water and pure aniline (the latter drawn from Thorpe's data, Trans. Chem. Soc., 1880) there were read the specific volumes of these substances at each of Alexéeff's temperatures; and from the combined information thus obtained, there was calculated the total volume of that quantity of the saturated solution at each temperature which contains 1 gram of aniline. This is what I have already called the specific solution volume. A slight error is involved by the fact that the volume of a solution is not exactly the sum of the volumes of its ingredients; but this error is necessarily small—too small to affect the general character of the curve or the nature of the lesson to be learned from it.

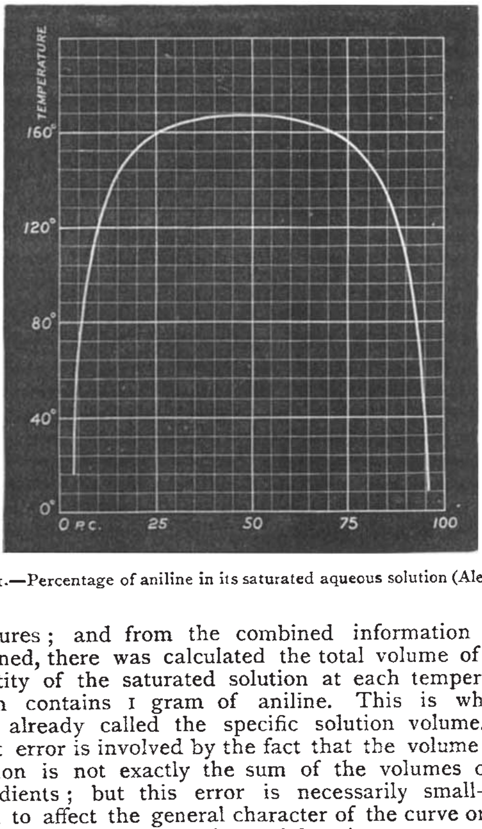

FIG. 1.—Percentage of aniline in its saturated aqueous solution (Alexéeff).

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[1] In order to save space, only the upper portion of the curve is here represented, as it shows all that is essential to the argument. Of the twelve experimental points, one appears to be somewhat misplaced; but this does not affect that part of the curve shown in the figure.

The specific solution volumes of the aniline, calculated in this manner, were found to be as follows :—

Temperature.	Specific solution volumes of aniline	
	In aniline layer.	In water layer.
8°	1·015	—
16	—	32·16
25	1·036	—
39	1·053	—
55	—	28·27
68	1·087	—
77	—	19·55
137	1·297	—
142	—	7·696
156	—	5·248
157·5	1·498	—
164·5	—	3·412

These specific solution volumes are represented as abscissæ in Fig. 2, with the temperatures as ordinates. For the sake of comparison, I have placed side by side with it a specific volume and temperature curve (Fig. 3) for pure alcohol and its saturated vapour, plotted from the experimental data of Ramsay and Young (Phil. Trans., 1886). The reason that alcohol was chosen is simply that the data were convenient to my hand.

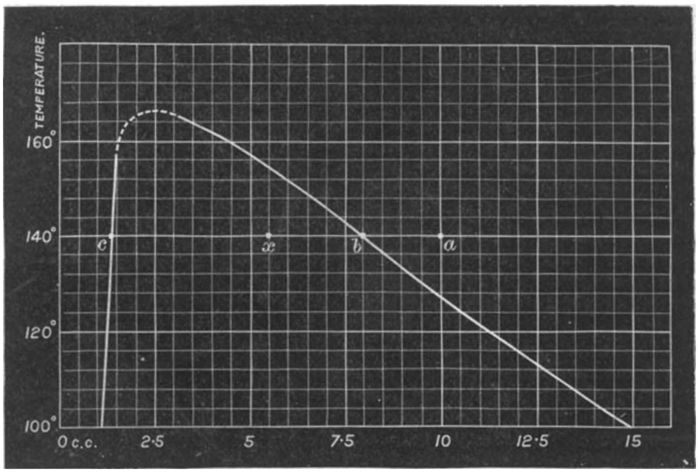

Fig. 2.—Volume of saturated aqueous solution containing one gram of aniline

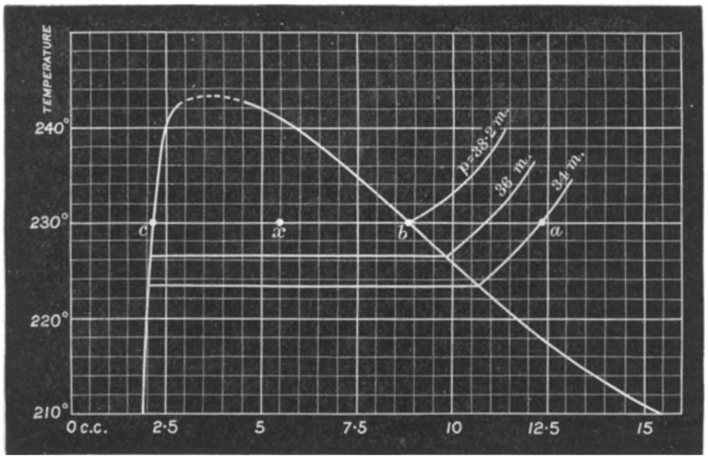

Fig. 3.—Volume of alcohol (liquid and saturated vapour) weighing one gram.

The two curves are strikingly similar in form and significance. In Fig. 3 we see the specific volume of liquid alcohol increasing slowly with rise of temperature, while that of the saturated vapour rather rapidly decreases. In Fig. 2 we see the specific solution volume of the aniline in the aniline layer slowly increasing, while that of the aniline in the water layer decreases more rapidly, with rise of temperature. In Fig. 3 we see that above the critical point the existence of liquid alcohol in presence of its vapour is impossible. In Fig. 2 we see that above the critical solution point the existence of an aniline layer in presence of a water layer is impossible. In Fig. 3 we see an inclosed area which represents those temperatures and specific volumes which are mutually incompatible. In Fig. 2 we see an inclosed area which represents those temperatures and specific solution volumes which are mutually incompatible. In Fig. 3 we see that any two points on the curve which correspond to equal temperature must also, from the nature of the case, correspond to equal osmotic pressure. In Fig. 3 some of the pressures are indicated, as this can be done from Ramsay and Young's data. In Fig. 2 the value of the osmotic pressures cannot be given, as they have not been experimentally determined. In Fig. 3 any point outside of the curve and to the right, as at *a*, corresponds to the state of unsaturated alcohol vapour, whose temperature, specific volume, and pressure are indicated—the last by the isobaric line which passes through the point. In Fig. 2 any point outside the curve and to the right, as at a, must correspond to the state of an unsaturated aqueous solution of aniline, whose temperature and specific solution volume can be read, and whose osmotic pressure could be indicated by an isobaric line, had we the data for plotting it. A little thought makes it evident, too, that such isobaric lines would follow the same general course as those shown in the alcohol diagram.

Now consider what must be the effect of gradually decreasing the volume of the unsaturated vapour in the one case and the solution volume of the aniline in the unsaturated solution in the other, while temperature is kept constant. In the case of the vapour (Fig. 3), the point a will pass to the left across lines of increasing pressure, until the vapour becomes saturated at b. Then, if the diminution of volume continue, a portion of the vapour will condense to the liquid state, or be transferred to c, while the rest remains saturated vapour at b. With continued decrease of volume, the proportion condensed will constantly increase, but there can be no alteration of pressure till all is condensed; and after that nothing but a very slight diminution of volume is possible without a lowering of temperature. Well, how are we to diminish the solution volume of the aniline in the unsaturated aqueous solution? Clearly by depriving the solution of some of its water, so as to leave the same quantity of aniline distributed throughout a smaller space. And what will be the result of doing this while temperature is kept constant? Evidently, as in the other case, the point a (Fig. 2) will travel to the left, across lines of increasing osmotic pressure, until it reaches b—that is, until the solution is a saturated one; and after that, if more water be abstracted, some of the aniline will be thrown out or condensed, not as pure aniline but as a saturated solution of water in aniline, so that two layers will now co-exist—the aniline in one having the specific solution volume represented at b, and the aniline in the other having that represented at c. This tranference from b to c will continue, as water is abstracted, until the ratio of residual water to aniline is just enough to give the whole of the latter the specific solution volume shown at c. At this stage the water layer will disappear, and only a saturated solution of water in aniline will be left; and after that only a very small volume change can possibly result from further abstraction of water, as the specific solution volume is already not far from the specific volume of pure aniline itself at the same temperature.

To complete the comparison of the two curves, let me point out that, just as we can from Fig. 3 calculate the distribution of alcohol between its liquid and its vapour layers under given conditions, so can we calculate from Fig. 2 the distribution of the aniline between the aniline layer and the water layer under given conditions. In the former case, if the total volume of a tube containing n grammes of alcohol, at, say, 230°, be $n \times x$, and if x be marked off (Fig. 3) between b and c on the line of that temperature, then (x, b, and c standing for the volumes which can be read off on the horizontal base line) $n\,.\,\frac{x-c}{b-c}$ is the weight of the alcohol in the vapour layer, and $n\,.\,\frac{b-x}{b-c}$ is its weight in the liquid layer, and the volumes of the two layers in cubic centimetres are $n\,.\,b\,.\,\frac{x-c}{b-c}$ and $n\,.\,c\,.\,\frac{b-x}{b-c}$ respectively, which are together equal to $n\,.\,x$. Just so also with the aniline and water mixture (Fig. 2). If $n \times x$ be the total volume of the mixture (both layers together) containing n grammes of aniline, at, say, 140°, and if x be marked off as it was in the other case, then $n\,.\,\frac{x-c}{b-c}$ is the weight of aniline in the water layer, and $n\,.\,\frac{b-x}{b-c}$ is its weight in the aniline layer, and the total volumes of the two layers are $n\,.\,b\,.\,\frac{x-c}{b-c}$ and $n\,.\,c\,.\,\frac{b-x}{b-c}$ respectively, together equal to $n\,.\,x$.

If the actual weights of aniline and water in the mixture be given, the value of x can be calculated with a very fair approach to accuracy by the method adopted in plotting the curve; and thus all the facts with regard to the distribution at any temperature can be obtained.

Now if it be remembered that this case of aniline and water is not an isolated one, but typical of many cases experimented on by Alexéeff, and if it be remembered also that there exists no direct experimental evidence to show that the law which governs these cases is not the general law regulating all simple solutions, it must I think be granted that the facts do somewhat strongly support the hypothesis of a critical solution point which I deduced in the first instance from the general theory of solution. It may be summed up as follows:—

(1) In every system of solution which starts with a solid and its simple solvent, the solid has a solution melting-point which is lower than its true melting-point. Above this temperature the system consists of two separate liquids, each of which is a saturated solution.

(2) These two liquids become one homogeneous solution at a temperature which depends on the ratio of the original ingredients. There is one ratio which demands a higher temperature than any other. This is the critical solution temperature, above which either ingredient is infinitely soluble in the other.

THEORY OF FUNCTIONS.[1]

II.

WE now come to Dr. Schwarz's contributions to the theory of functions, which relate to the two theorems stated, and we begin with those which relate to the conform representation of various surfaces on a circle.

In the paper "Ueber einige Abbildungsaufgaben," we learn that these investigations date back to the time when the author was a student at Berlin. A fellow-student, Herr Mertens, observed to him how curious it was that Riemann had proved the existence of a function which would give a conform representation of the surface of a figure like a triangle on that of a circle, whilst the actual determination of the function in form of an expression seemed, on account of the discontinuities due to the corners, to be beyond the present powers of analysis. Dr. Schwarz thereupon tried to work out a special case, and selected the representation of a square on a circle, or, rather, first on the half of the plane which is bounded by a straight line; for the circle can be conformly represented on this half-plane by aid of reciprocal radii.

He next looks for a function which has such discontinuities as are introduced by the corners of the square. The right angle has to be represented by an angle of twice the magnitude, and therefore here the representation cannot be similar in the smallest parts. The known properties of the exponential function help at once to find it. This function is thus obtained by a happy guess, not by prescribed rules. It still contains an infinite number of constants. To determine these it is observed that if $u = f(t)$ gives a conform representation of a surface T in the plane t on a surface U in the plane u, then also will $u' = C_1 u + C_2$ gives such representation, only the corresponding figure U′ is drawn to a different scale, and placed in a different position in the u plane. On differentiating this equation with regard to

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[1] "Gesammelte Abhandlungen." Von H. A. Schwarz. Two Volumes. (Berlin: Julius Springer, 1890.) Continued from p. 323.